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then taps the blocks with the fifth cube in a certain definite order and at a certain definite rate (about one tap per second), always beginning with the cube at the child's left or the examiner's right if he is facing the child. He then lays the fifth cube down in front of the child, equidistant between the third and fourth cube, but nearer to the child, and says, 'Do that.' The blocks were tapped in 12 different ways, designated lines A-J. The complexity of the pattern which can be followed is the index of performance in the test. Curves are given showing the percentages of correct responses for each age and how the patterns ("Lines") compare with each other in difficulty. These curves are quite significant for the author. In defining the age limit of a test he would require about 60% passes but considers that beyond that the best age for placing a test will depend on the shape of the curve showing a percentage of passes at each age. The correct passing of three of his lines, BCD, is delayed until about the fourteenth year. The test may be also evaluated in terms of the number of lines passed correctly at each age. The results gained in this way are about the same as by considering special groups of lines, which is his first described method. In 20 cases the Binet ages were available for comparison and the author considers it surprising that any one test should come so near the results arrived at by a whole series of tests. Two definite types of errors, perseveration and reverse order, are noted. Normal children on the whole do slightly better in the cube test than the feeble minded of corresponding mental age. In summary he gives performances in the test, either by the group method or the actual number of lines method of scoring, which correspond to mental levels of 5, 6, 7, 10, 14 and 16 years. He considers that the child should be credited with the age at which the most difficult combination is passed.

The Adequacy of the Laboratory Test in Advertising. H. F. ADAMS. *Psychol. Rev.*, 22, 1915. pp. 402-422.

The feasibility of predicting through a simple experiment the relative amount of business which different advertisements will bring in is indicated in the writings of Strong and Hollingworth. The general conclusion is drawn that the laboratory test is a satisfactory preliminary in advertising. It was thought that mail order advertising would be worth studying in this connection. A satisfactory measure of the pulling power of an advertisement is not easy. Factors of follow-up, salesmanship and the like seem to make the number of inquiries per insertion the fairest measure. The laboratory tests do not determine whether an advertisement is absolutely good or bad, but only relatively to other members of a series. The figures quoted from the author's experiments show "simply chance resemblance between the results of the laboratory test and the average number of inquiries per insertion, and very little better than chance resemblance between the laboratory test and the business test where profits are used as a measure. . . . With a mail order business it is possible to get returns which are extremely accurate so such advertisements would make the best material for laboratory tests if such tests would only work." College students are however not fair representatives of the mail order purchasing public. The author has been led to question the application of the order of merit method to advertising problems and the previous experiments seem open to attack on several sides. The elaborate keying necessity is impossible with many commonly used articles. Further, the results obtained from laboratory testing are as though the whole hundred per cent of readers were tested instead of

the ten per cent who would on the average be interested enough to answer the advertisement. Further, there is the difficulty of any person's telling experimentally just what his actions will be.

Die Abwehrfermente Abderhalden's in der Psychiatrie. By F. SIOLI. *Archiv für Psychiatrie*, 55, 1915. pp. 241-271.

A comprehensive digest of the literature is presented, and the author appends some comparative observations, using an organ in different stages of disease. There were 15 cases altogether, of whom 10 were senile dementes. The author concludes from the literature as well as from his own observations that the results from the Abderhalden and allied methods are not yet sufficiently clear to be used for purposes of diagnosis and prognosis or pathogenetic theory.

F. L. W.

Proceedings of the Society for Psychical Research, Volume XXVIII
Glasgow University Press, 1915.

The twenty-eighth volume of the Society for Psychical Research (1915) devotes its 657 pages to a "Study of the Psychology of Mrs. Piper's Trance," by Mrs. Henry Sidgwick. According to a list printed at the beginning this brings the total number of pages published by the Society on Mrs. Piper to 2812, the first one coming out in 1890, and yet so dubious is the outcome that Mrs. Sidgwick concludes: "To sum up very briefly my own conclusion about Mrs. Piper's trance, I think it is probably a state of self-induced hypnosis in which her hypnotic self personates different characters either consciously and deliberately, or unconsciously and believing herself to be the person she represents, and sometimes probably in a state of consciousness intermediate between the two. In the trance state her normal powers transcend in some directions those of her ordinary waking self, as is often the case in hypnosis. And further—what makes her case of great importance—she can obtain, imperfectly and for the most part fragmentarily, telepathic impressions. . . . It seems to me impossible at present to prove any theory on the subject." But at the beginning, Mrs. Sidgwick expresses herself thus in capitals (p. 6): "To prevent misapprehension, I am anxious to say emphatically at the very beginning of my discussion that I have no doubt whatever that knowledge is often exhibited in the course of Mrs. Piper's trance utterances which can only have reached her by some supernormal means—by which I mean otherwise than through the ordinary channels of sense," and she inclines to believe that this includes not only telepathy from the living but from the dead. Again, she recognizes that the "communicators" cannot be taken at their face value. "All these points being taken into account, grave doubts are thrown on the genuineness at any time of the dramatic presentation of the communicator. Veridical communications are received, some of which, there is good reason to believe, come from the dead and therefore imply a genuine communicator in the background. But the dramatisation of even genuine communicators, with the whole dramatic machinery employed, is probably merely dreamlike."

If such a wavering stand is the outcome of over thirty years of work, by various individuals, who were from the beginning at least sympathetic with the spiritistic hypothesis, on the "medium" who is generally admitted to give the best returns with the least suspicion of deception, what is the probability of spirit communication?

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